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The Place of Agricultural Research in the Development of Sub-Saharan Africa*

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Summary. — Outside sub-Saharan Africa (SSA), agricultural research (AR) yields excellent returns. Why does SSA get so little from its major AR effort? Why do its AR scientists cost more, yet produce less?

Smallness (of countries and research stations), dispersion, and high turnover make it hard to attain a "critical mass" of national AR scientists. To remedy this, they could concentrate on a few problems and crops — yet they have neglected many of the most important, e.g. cassava, and overstressed export crops. In other ways, too, European biases have distorted African AR. Socioeconomists, moreover, have entered research design too little and too late.

Above all, current domestic funds have been too scanty and unreliable to adequately support international and capital-account AR efforts. This lack of steady commitment illustrates AR's need for direction from clearer agricultural policy — based on radically improved information, and recognizing SSA's dramatic rise in labor/land ratios. Guidelines for such policy are indicated; within these, a formalized and poverty-oriented AR design system is suggested.

1. PARADOX: RESEARCH PAYS, AFRICA SPENDS, RETURNS LOW

(a) *Introduction*

This paper is confined to agricultural research (AR) in sub-Saharan Africa (SSA), defined to exclude the Republic of South Africa.[1] Generalizations about SSA proper are difficult for three main reasons.

(i) SSA contains several major agro-climatic *zones*; many countries contain more than one.[2] Considerably less AR attention has been given to tropical and desert zones than to the sub-tropical zone (Boyce and Evenson, 1975, pp. 2–3) where agricultural growth has been faster.

(ii) *Polities* vary from those where the transmission of existing AR outputs is hampered by law-and-order factors (Uganda) or extreme price distortions (Ghana),[3] to those with promising, serious AR efforts constrained technically (Kenya).

(iii) *Economies* vary in export orientation, agricultural shares in workforce and GNP, land/labor ratios, and agricultural priority.

Nobody would generalize about AR for Asia — from Lebanon and Jordan, through Nepal and both Punjabs, to Central Luzon and Mindanao. Are such generalizations about SSA merely testimonies to ignorance — all cats are grey in the dark?

Three general statements can, nevertheless, be made. Despite the pathbreaking experiences of (say) the Punjabs, Sonora, and Central Luzon — and despite the African precedent, now 25 years old, of SR-52 hybrid maize — most of SSA now offers smallholders no dramatic, immediately applicable new technology that might, with plausible increases in output/input price ratios, or in person/land ratios, safely and substantially increase the profitability of food farming over large areas. While this is so, the elasticity of total farm output to currently recommended policy changes, including price changes, can seldom be very large. More or better AR is necessary, although seldom sufficient, to remedy this.

(b) *The paradox and possible resolutions*

The policy issues can be addressed by seeking to explain a paradox. Rates of return to AR have been shown to be very large. By the standards of the developing world, SSA appears to be spending a good deal on AR (and to be supporting it with unusually high levels of extension).[4] Yet, by

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those same standards, agricultural growth in SSA, except for a few countries, has been notoriously slow. Moreover, SSA is unlike other parts of the world in that the success of a nation's agriculture does not seem to have been strongly linked to its level or growth rate of AR outlay or scientist numbers.

Section 2 outlines the paradox, and examines the explanations.

First, there is less AR in most of SSA than there seems to be. Many, probably most, SSA countries are paying for — and getting — much less AR than the crude numbers (dollars, scientific person-years) indicate.

Second, an even more important explanation of the paradox (that apparently substantial AR leads to little extra farm production) lies in reduced impact per unit of AR, due to its scale, relevance, and policy frames in SSA. One group of problems concerns *critical mass* (Section 3). There seem to be scale-economies in research-station size, to well above the sizes achieved in most of the SSA countries.[5] Yet the difficulties of SSA countries, mostly poor and small, in attaining critical mass are aggravated by high turnover; by "loss" of AR benefits to nearby countries without regional coordination; and by dispersion of scientists among stations and programs.

This last problem arises partly out of efforts to solve a second group of problems, those of *relevance* of centralized AR to local conditions (Section 4). Partly, this is due to lack of congruence (Boyce and Evenson, 1975, pp. 83–98; Judd, Boyce and Evenson, 1983, pp. 23–28) between the output-mix and the research-mix, even allowing for different prospects of success in different types of work. Partly, it reflects inadequate integration of economic and social analysis into agricultural research, especially at the design stage. The overseas orientation of much SSA research, especially post-doctoral, cannot help either. In this context the "farm systems" approach, while useful, should not be seen as a panacea, especially if it involves "rapid rural appraisal" of areas where planners and researchers are ignorant.

That ignorance brings us to the main reason why SSA agricultural research has not contributed more to output: the absence of a proper *policy framework* (Section 5). This gap explains the shortage of basic facts on farm output, especially smallholder food output — and even, to some extent, about farm research. Is "research policy," then, the key? In one sense, no: little will be achieved by persuading SSA countries to adopt a blueprint that centralizes agricultural research upon a high-powered research institute or interdepartmental committee. In a second sense, again no: it is agricultural policy (on food strategies, nutrition and income-distribution, information systems, labor-intensity, irrigation and water management, and above all agriculture's share of real resources) that is required, before research policy can help. Yet, in a final sense, research policy is the key (Section 6). It is possible to outline research contents, career structures, and priorities that — given adequate real indigenous resources and a roughly feasible agricultural policy — will greatly increase the chances that those resources will achieve the output and distribution goals of the policy. So far, research policy has been mainly discussed in terms of (1) organizational options, where there is little knowledge and much opinion; (2) how to raise the expected benefits from research by changing its content, where Asian experience does provide some guidance; (3) — far too little — how to cut costs, including uncertainties and delays. Transfer of research experience from other developing areas may be even more relevant to cutting costs than to increasing benefits.

2. SOME EVIDENCE ABOUT THE PARADOX

(a) *High returns to agricultural research*

In 1975, 10 examples of direct cost-benefit analysis of national AR, and 11 studies relating growth of farm output to several sources including national AR, were collated. Internal rates of return in the former group ranged from 20% (US poultry) to 60% (Indian sugarcane) and 45–93% (Mexico, various crops). The seven studies from developing countries showed somewhat higher returns than the studies from developed countries, and a parallel study "estimated marginal internal rates of return of 42% to technologically oriented research in developing countries and 21% in developed countries" (Boyce and Evenson, 1975, p. 110, fn.). However, there was not one study of returns in SSA (*ibid.*, pp. 103–107).

By 1982, 50 such studies — still not one in SSA — could be collated. "Average annual rates of return for these programs are slightly less than 50%, only four showing returns of less than 20%. Eight studies in Asian developing countries showed average returns of 44%, and the 14 quantifiable returns in Latin America averaged 47%" (Pinstrup-Andersen, 1982, pp. 102–104). Although there may be a tendency to select the more successful crop-specific programs for evaluation, the tables include several "studies of complete national AR systems" giving "returns similar to those obtained from more limited research programs. Thus the upward bias caused by a possible bias of programs to be analyzed may not be very great" (*ibid.*, p. 104).

(b) *Large African agricultural research outlays*

The size of AR in SSA, compared to agricultural output and even agricultural production, *seems* to be quite large. In 1974, public sector agricultural research expenditure was 1.12% of agricultural output in western SSA, 1.63% in eastern SSA, and about 1.45% in southern SSA.[6] Figures for other developing regions were substantially lower, e.g., 0.31% in South Asia and 0.49% in Southeast Asia.[7]

Comparisons for 1980 are available only for a few less developed countries (LDCs). They show a similarly high-spending performance on SSA research, at each income level (Table 1). Constructed estimates for major regions as a whole (Table 2), while apparently using a more comprehensive definition of AR scientists,[8] confirm this, as do other implications of Table 1.

Great skepticism about the numbers for LDCs, especially in SSA, in Tables 1 and 2 is warranted. First, for most of SSA, numbers for cropped area and output in subsistence agriculture — typically engaging over half the population — are little more than guesses. Second, the data for scientists engaged in national public-sector AR often show major conflicts[8], even from the same source: for example, ISNAR estimates the number in Upper Volta around 1980 variously at 15 (Oram and Bindlish, 1981, p. 80) and 123 (Antoine *et al.*, 1983, p. 34); some time-series data are also surprising.[9] We return to the question of agricultural information, and its role in research planning, in Section 5.

Whatever allowances need to be made for bad data, however, we have a paradox. Returns to AR are extremely high, especially in LDCs (though the abundant evidence is silent on SSA). Tables 1 and 2 show agreement, among several sources, that public-sector[10] outlay per unit of GDP, of land area, and of agricultural population[11] was much higher in the 1970s in SSA than in South or Southeast Asia. Yet agricultural production grew far more slowly for most of SSA than for most of South, and almost all of Southeast Asia (World Bank, 1984, pp. 90, 220). There is no correlation between substantial (or fast-growing) AR and good agricultural performance among countries in SSA, as there is, for example, among India's Districts (Evenson and Kislev, 1976; cp. Oram and Bindlish, 1981, Annex 2B). Paradox deepens when we observe the rapid growth of national public sector AR outlays in SSA — more than tripling as a share of agricultural GDP in West Africa, and more than quintupling in East Africa, in 1959–80 — alongside sharp deceleration in agricultural growth (Judd *et al.*, 1983). Since real research outlays (per unit of land, farmwork, or value-added) in most of SSA have been well above levels in other developing regions at least since the early 1970s, the paradox of slower growth cannot be explained mainly by the lag between research initiation and its results.

Of course, even if all research in LDCs is of uniformly high quality, and of similar cost per unit of "results," research is not *sufficient* for growth. The constraints placed on agricultural growth in SSA by the physical, climatic, trade and policy environments — often bad, worsening, or unpredictably fluctuating — are dealt with in much other published work. This paper concentrates on the features of the research process, and of policymaking towards it, that might help explain the paradox. The dichotomy "research-environment" cannot be pushed too hard, though. Good research aims at suitability for environment — robust seed varieties even if the rain is moderately late, or the fertilizer composition moderately inaccurate (should these be typical SSA risks).

It is, in any case, fairly clear that national agricultural research in much of SSA is not giving value for money. Large outlays are buying modest numbers of scientists, often underqualified, who are producing extremely modest results by world standards.

(c) *Research costs high, but output low*

Tables 1 and 2 show the high cost of "scientific person-years" in SSA. A few figures put this cost into sharper perspective. Compared to the low-income South Asian countries (India, Pakistan, Bangladesh, Sri Lanka and Nepal) the six low-income SSA countries with available data for 1980 spent 1.8 times as large a share of (alleged) agricultural GDP on national agricultural research (Table 1; the regional estimate in Table 2 suggests an even greater disparity). However, in 1980 they paid 3.2 times as much per scientist-year (Table 1; the regional disparity estimated in Table 2 is somewhat less). Nevertheless, in 1974, SSA obtained only 55% as many "standardized publications" per scientist-year (Boyce and Evenson, 1975, p. 42). Thus, despite a much larger research effort relative to agricultural GDP, the low-income SSA countries with recent figures appear to have obtained barely 30% as much national "research output," as indicated by

Table 1. *National public sector AR*

	Number of countries	Research expenditure ($m 1975)	No. of research scientists	US $ (1975) outlay per: $10,000 agric. GDP	Thousand persons	Cropped hectare	Scientists per: $10m agric. GDP	Million persons	100,000 cropped hectares
Low income:									
South Asia	5	140	12,300	32	156	0.7	2.8	13.7	6
SSA	8	45	1,200	58	438	1.5	1.6	12.0	4
Middle income:									
Southeast Asia	9	105	6,000	35	344	2.1	2.0	19.5	12
North Africa, West Asia	2	4	200	26	339	0.6	1.3	17.5	3
SSA	4	105	1,700	67	1,111	3.3	1.1	18.5	5
Latin America	12	50	1,700	50	649	2.4	1.5	19.5	7

Source: Oram and Bindlish (1981), p. 28. Figures in the first three columns are rounded. Column for "outlay per cropped hectare" amended to correct decimalization error in source.

Table 2. *National public sector AR: Global estimates, selected regions*

Region/subregion	Expenditures ($m of 1980) 1959	1968/71	1980	Scientist person-years 1959	1968/71	1980	Expenditures per scientist-year ($000 of 1980) 1959	1968/71	1980	Expenditures per $10,000 agric. GDP ($ of 1980) 1959	1968/71	1980
West Africa	44	92	206	412	952	2,466	108	97	83	37	61	119
East Africa	13	49	75	221	684	1,632	58	72	46	19	53	81
Southern Africa (excl. RSA)	2	6	17	146	191	299	12	41	58	n.a.	n.a.	n.a.
West Asia (excl. Israel)	13	52	95	287	1,223	1,699	45	42	56	n.a.	n.a.	n.a.
South Asia	32	73	191	1,433	2,569	5,691	22	28	34	12	19	43
Southeast Asia	9	37	103	441	1,692	4,102	20	22	25	10	28	52
East Asia (excl. China, Japan)	6	24	50	637	1,120	1,591	10	22	32	n.a.	n.a.	n.a.
China	54	502	644	1,250	12,250	17,272	43	41	37	9	68	56
North Africa	21	50	62	590	1,122	2,340	35	44	27	31	62	59
Temp. South America	31	57	80	364	1,022	1,527	85	56	53	39	64	70
Trop. South America	35	129	269	570	2,698	4,840	61	48	56	25	67	98
Central America, Caribbean	14	30	113	491	1,160	2,167	28	26	52	15	22	63
Northern Europe	95	230	410	1,818	4,409	8,027	52	52	51	55	105	160
Eastern Europe (excl. USSR)	196	436	553	5,701	16,009	20,220	34	27	27	n.a.	n.a.	n.a.
Japan	135	498	684	7,200	12,600	15,671	19	39	44	n.a.	n.a.	n.a.
North America	669	1,221	1,336	6,690	8,575	10,305	100	142	130	84	127	109

Source: Judd, Boyce and Evenson (1983), pp. 5, 11, 15, 60–61.

standardized publications, per unit of agricultural GDP as did the South Asian countries.[12] Several specialists at a 1986 IFPRI meeting concurred that the cost of doing a comparable piece of research was at least three times as high in SSA as in South Asia; for low-income countries "30 percent as much research *output* per unit of agricultural GDP, despite 1.8 times more *outlay* per unit" suggests an even worse relative performance. The comparisons are no better (as compared with *East* Asia) for middle-income SSA.

Clearly, this has much to do with the environments — geoclimatic, economic policy, research organization — for effective national agricultural research in SSA. It does not seem to be explicable by three possible glosses on the above figures. First, it is not the case that national agricultural research in SSA is getting a worse ratio of PhDs to less-qualified scientists than in South Asia (Oram and Bindlish, 1981, pp. 36–37).[13] Second, it is not the case that SSA researchers are supported by less extension than in other developing countries — rather the reverse (Judd *et al.*, 1983, p. 11). Third, it is not the case that SSA suffers, by comparison with other developing regions, for want of supportive work in Africa, for example under the aegis of the Consultative Group on International Agricultural Research (CGIAR). By the end of 1979, the CGIAR institutions in SSA — IITA (Ibadan), ILRAD (Nairobi), and ILCA (Addis Ababa) — had spent 37.8% of the cumulated core outlays for all CGIAR institutes on capital budgets, and 27.6% on current budgets. In 1980 these institutions were joined by WARDA (Monrovia) and the proportions for that year rose to 38.5% and 34.8% respectively; of total 1980 CGIAR core outlays ($132.5 mn), the four SSA centers spent 31%, almost as much as the 32% for the three Asian centers (IRRI, Los Baños, Philippines; ICRISAT, Hyderabad, India; ICARDA, Aleppo, Syria), which related mainly to an agricultural population over twice as large. Moreover, the remits and operations of the CGIAR's Africa-based institutes are far more closely confined to their base continent than is the case for Asia-based institutes (Pinstrup-Andersen, 1982, pp. 85–88, 94–95). On top of this, national agricultural research in Francophone SSA is complemented by a massive and costly French-based input of international research centered on ORSTOM and GERDAT (von der Osten *et al.*, 1982, esp. p. 18), for which there is no Asian parallel.

(d) *Research content vs. local commitment*

So part of the explanation of the SSA agricultural paradox — high national AR outlay, slow agricultural growth, yet world-scale evidence of good agricultural returns to research — is that most of SSA is getting exceptionally little "real" national research output per scientist, and paying heavily for each scientist: and this despite unusually high support from extension, and from international research systems. One conclusion is certainly that, since "countries will respond to lower prices of national scientific resources . . . the issue of training scientists at low cost in national programs now deserves much greater attention from aid donors" (Judd *et al.*, 1983, p. 47). We shall see, further, that so-called "national AR" in much of SSA — though there are important exceptions, such as Kenya — reflects *foreign* money, personnel, and intellectual commitment to a much greater extent than in other developing regions. The blame does not attach mainly to SSA scientists, but to policy environments, structures and *contents* of research that render national commitment ineffective and therefore unattractive.

3. "CRITICAL MASS"

(a) *Problems of smallness*

Part of the reason why many African governments — as opposed to aid donors — find AR unattractive (and we know that governments tend to buy less AR where it is more expensive: Boyce and Evenson, 1975, p. 121) is the group of problems associated with *critical mass*. Worldwide, countries with below $400 of GDP per person in 1974 located 49% of their scientists in experiment stations with over 21 persons; the proportion for better-off countries was 61% (Boyce and Evenson, 1975, p. 83). Poor communications *within* LDCs make the dispersion of scientists in SSA even more damaging to prospects of achieving a critical mass; so does the fact that, because fewer scientists are highly trained (and because technicians and administrators are scarce), much research time in each station is diverted to technical and administrative duties.

Compared with South Asia, too, the small populations of most SSA countries — together with the fact that a typical such country combines agricultural diversity with linguistic specificity — aggravate the problem even further. Small populations, plus the "fixed cost" imposed upon each research station by its share in administering and maintaining a national system, damage prospects for reaching a critical mass directly, but the indirect effect is more serious. Each small country's government must fear, rightly (see Boyce and

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Evenson, 1975, pp. 101–102, for evidence), that its own spending on research will produce mostly unrequited benefits for foreigners. Regional research cooperation has its own costs, both if nations fall out (as in East Africa) and if free-riding has to be policed. The need to incorporate several disciplines, socioeconomic as well as scientific, if AR is to produce results that benefit smallholders (Section 4) also carries a double burden: it directly raises the required critical mass, and it does so indirectly by imposing upon researchers costs of communication to non-specialists.

(b) *Dispersion and turnover*

In these extremely difficult circumstances, there are two possible ways to ease the problem. The first is to reduce staff turnover — and changes in programs, regions, and tasks; a critical mass could more readily be achieved at a 15-person research station where all workers remained to concentrate on two programs for five years, than at a 30-person station with high turnover and/or many (or rapidly changing) tasks. The second is to reduce the number of research stations. Clearly the second approach has drawbacks, so one would expect great concentration on the first.

Unfortunately, most SSA countries have avoided the first approach. Kenya is a particularly telling case, because both its AR and its medium-term agricultural growth have been well above the SSA norm (Taylor *et al.*, 1981, p. v). Yet a USAID report of September 1977 documented "the loss of more than 58 research scientists [out of those] working three years earlier" (*ibid.*, p. 77), i.e. about one in four (Boyce and Evenson, 1975, p. 174; Judd *et al.*, 1983, p. 60; Oram and Bindlish, 1981, p. 89). This is not due mainly to high expatriate turnover: average length of employment in the research division of the Ministry of Agriculture was 2.5 years for Kenyan personnel and 3.5 years for expatriates (Taylor *et al.*, 1981, p. 80). Faster indigenization is indeed, in the longer term, probably necessary to reduce turnover; but it is not sufficient. On a crude interpretation of these Kenyan data it would actually make turnover faster in the short term (although a more careful analysis might reveal that many of those who "drained" abroad, or into non-research activities at home, did so because of frustration that expatriates in top jobs blocked promotion).

Not all turnover is loss. Some is due to higher training, or transfers to research in the private sector, to (doubtless progressive) farming, or to academic research or management. One does, however, have the uncomfortable suspicion that most turnover is into jobs, often abroad, that do little for AR in SSA. Certainly, much occurs because

> AR scientists and their role in national agricultural development lacks recognition[;] the research cadre [is paid less] than people with comparable training and experience in other components of the agricultural sector; [and] the present scheme of service does not provide for scientific career development, recognition and reward for research productivity. (*ibid.*, pp. 80–81)

Pay is not the *main* problem; most Asian AR scientists' "pay gaps," to Western salary levels, are well above comparable African scientists', yet drains are usually much less. Rewarding work, proper support, and career structures are the main problems. If the career-based causes of high turnover, diagnosed in the last citation, apply even in Kenya, they apply much more forcibly in most of SSA, and are richly documented in various ISNAR reports (Section 6). Anyway, even where the causes of turnover from public AR are benign and where the recipient sectors benefit greatly, the problem of lost critical mass remains. The aggravation is even more extreme than the above figures indicate, because — apart from turnover out of public AR service — there is often very high mobility within it, between research stations.

Moreover, high turnover among, and out of, public sector AR stations is damagingly synergistic with the often large number of such stations. If we remain with Kenya, "research on priority [food] crops is undertaken principally in 42 national and regional sub-stations"; there are also separate commodity research stations for sugar, coffee (four), tea and cotton, and for livestock, animal health, and forestry. Many of these stations suffer personnel shortages (*ibid.*, pp. 32–33, 36–37, 54 and Annex 10):

> Resources are considered inadequate in 25 research establishments, and very inadequate in about 15 . . . there are more research stations than can be adequately staffed . . . Sub-stations should be managed by [technicians, and scientists concentrated in a few regional and national centers]. It seems wasteful . . . to locate one or two young research scientists at [each of many] sub-stations where they are likely to receive little or no guidance and where their chances of being productive are minimal. (*ibid.*, pp. 54–55)

Such "spreading thin" not only worsens the damage done by high turnover, but it also induces it:

> A main technical requirement for strengthening Kenya's agricultural research system is an im-

> mediate injection of qualified and skilled personnel who could lead training for young graduates entering the profession. Lack of such guidance has been instrumental in the loss, through despair, of good potential scientists. (*ibid.*, p. 83)

But the costs of this "injection" will be prohibitive, unless it can be concentrated — alongside the young entrants — upon a much smaller number of stations.

The problem of dispersion among stations has been exemplified for Kenya because this case is relatively favorable. In the Ivory Coast, "la balkanisation actuelle de la recherche agronomique en un nombre élevé d'institutions autonomes, de nationalités, statuts, tutelles et modes de financement" has been very imperfectly remedied by the Ministry of Scientific Research, created in 1971 (von der Osten *et al.*, 1982, p. 20). In Malawi, in 1982, apart from the three main stations sharing 60 scientists, another eight minor stations shared 12; once again, "the spreading of meager resources across an inefficient network of stations exacerbates the effects of inadequate resources" (Gilbert *et al.*, 1982, pp. 12–13, 28).

Yet dispersion addresses real issues, especially those of diversity among environments. Concentration of most resources into the likeliest place, Ministry buildings in or near the capital city, is likely — as at ARS Sebele, Botswana, after the phasing out of Mahalapye and other sub-stations — to produce atypical physical environments and inadequate outreach. In Rwanda, an expert team recommended a search for greater relevance via "des stations d'expérimentation et d'essai dans chacune des grandes zones agro-écologiques du pays" (Contant *et al.*, 1982, p. 13) plus a central station. There are three or four such zones (*ibid.*, p. 10), and scientific staffing is supposed to be multidisciplinary (*ibid.*, pp. 13–14). Yet AR in Rwanda's public sector in 1980 (*ibid.*, pp. 79–84) mustered only 24 scientists (Judd *et al.*, 1983, p. 60). By 1982, the exodus of Belgians had reduced the numbers with the Institut des Sciences Agronomiques de Rwanda to 18: seven at the Rubona central station, three (including the only two foreigners) at Ruhande Arboretum, two each at Songa (animal selection) and Karama (crop and animal trials), and one each at Tamira (pyrethrum) and Rwerere (high-altitude fruit, vegetable, and goat research!).

It is always tempting to improve regional representativeness by multiplying research stations; but it tends, in SSA, to worsen an already very serious lack of critical mass at each station. An obvious idea is to create microcosms of ICRISAT, i.e. national stations located at the borders of two or more agroclimatic zones. Simply to multiply tiny stations, each with few scientists and little or no senior supervision, is a hopeless approach Concentration of stations, however, risks even more exclusion of remote (and often very poor) areas, and even more submersion of rural research in urban priorities. *Like many issues of research organization, the issue of "critical mass" is unlikely to respond to neat, general blueprints, applied rapidly by teams of visiting experts, and claiming to provide general answers to major administrative questions.* (The analogy to another macrosystemic will-o'-the-wisp, overall Administrative Reform, is striking: see Schaffer, 1973.) Much more promisingly:

(i) Simple, unpretentious improvements in O. and M. could increase critical scientific mass via lower turnover and fewer techno-administrative diversions;

(ii) Attention to the *content* of agricultural research could help. Across a great variety of organizational forms in LDCs, some *sorts* of crop and animal problems seem to respond to research. Concentration of scientists on these research issues would also help to reduce the current dispersion, by increasing the critical scientific mass applied to fewer issues (on multiplicity of programs in Malawi, see Gilbert *et al.*, 1982, p. vii).

4. CONTENT: CONGRUENCE, FOREIGNNESS, SOCIOECONOMICS

(a) *Extreme incongruence between research and crop mix*

Dispersion of scarce scientists among many, distant stations[14] is a bad way to make AR relevant to the problems of diffuse, extensive, diverse agricultural countries in SSA; but it recognizes the relevance problem, just as the concentration proposals recognize the critical-mass problem. Are there better ways to increase the relevance of AR to raising SSA's agricultural welfare, without reducing — indeed perhaps while increasing — sustained, collaborative scientific concentration on central issues? One approach is to achieve better "congruence" between output-mix and research-mix. A second is indigenization of AR personnel, priorities and purses. A third is to combine socioeconomic with agricultural-science analysis. A fourth is to switch applied AR into "farm systems" as well as, or to some extent instead of, individual commodities (Collinson, 1982).

"The simple rule [of congruence, that] the ratio of the research expenditure for a commodity to [its] economic value of . . . should be equal for all commodities[, is] based on the 'plasticity of nature' [which is] over the long term [reasonable]" (Boyce and Evenson, 1975, p. 84). By "plasticity" they mean that, whatever the commodity, "efforts to uncover the secrets of nature" are, more or less, "equally productive no matter where the effort was directed." They recognize (a) that a country should research at levels below those indicated by the congruence rate into products heavily researched by "environmental neighbors"; and (b) that products in price-inelastic demand might justify lower research/output ratios than for heavily-traded commodities for which "demand will be quite elastic" — and below-congruence ratios for "heavily protected commodities" (*ibid.*, pp. 118, 120).

The congruence rule — subject to modification (a) above, to commonsense refusal to keep on at products or soils clearly unresponsive for scientifically demonstrable reasons, and to an important distributional caveat — seems a useful rough guideline for SSA. Modification (b), however — as an explanation or justification of the markedly superior research outlays, facilities and organizing efforts in traded or price-elastically demanded products than in subsistence products (see, for example, Taylor *et al.*, 1981, p. 35, on Kenyan coffee) — appears to be dubious, for four reasons. First, if benefits from research into Product X are largely passed on to *domestic* consumers (because X is not traded internationally and demand for X is price-inelastic), that seems a bad reason to curtail research. Second, the costs of adjustment for domestic producers in such a case will depend mainly on cross-elasticities of supply — as determined by factor availabilities, production functions, technologies, and relative factor prices — and not on own-price-elasticity of supply; for example, even if sorghum is little traded internationally and in price-inelastic demand domestically, the costs of switching land and other factors to next-best uses are likely to be small, but if a tree crop is in question producers' adjustment costs will be much larger. Third, modification (b) would appear to justify low levels of research into low-value, high-transport-cost subsistence crops, eaten mainly by their own growers — cassava, sweet potatoes, some grain legumes — that in SSA have suffered most from "sub-congruence" ratios (York, Miller, Dalrymple *et al.*, 1977, p. 51); yet these crops, being eaten largely by those who grow them, do not neatly generate rules dependent on market-price elasticities. Fourth, traded products, especially the export crops that still get the lion's share of SSA research, are often price-inelastic for growing and researching countries with big market shares, and even more often for growing and researching LDCs as a whole. Tea has a world price-elasticity of about −0.3; the dramatic increases in yield due to clonal varieties meant that tea research in the 1950s in India and Sri Lanka, each with a world market share then above 0.3, benefited rich-world consumers but damaged those countries. Kenya's coffee research, by glutting markets with similarly low price-elasticities, may well transfer income from poor to rich countries.[15]

The distributional caveat is that the congruence rule, in assuming "plasticity of nature" (in respect of marginal as well as average long-run expected productivity of research across commodities), finesses the question of whether extra product *values* are correctly reflected by relative *prices*. Many would give greater weight, per unit of extra output, to products produced or consumed — preferably, in view of possible price-inelasticities, both — by poor people. That would imply below-congruent research/output[16] ratios for others, notably (despite modification (b) above) for estate-based export crops.[17]

Do SSA commodity compositions of AR — allowing for distribution, for non-responsive commodities, for "free" foreign research, and for the wish not to pass on research benefits to "price-inelastic demanders" in rich countries — achieve enough relevance-by-congruence? Table 3 reveals a tendency to concentrate research for a crop upon places where it is *not* a locally-consumed or poor person's product (compare Asian and African data for rice and wheat: much more mass-consumption crops in Asia, much more prestige and high-research products in Africa). In general, Africa reveals much greater disproportions, in respect of emphasis upon AR into exported commodities and the products of richer farmers and urban consumers, than does Asia or even Latin America. Judd *et al.* (1983, p. 27) develop a measure of overall commodity congruence, between output-mix and AR-outlay-mix, for 26 LDCs, including six in SSA. Confirming the hints of Table 3, this measure shows that the SSA countries are among the less congruent overall: Ghana ranks 11th out of 26, Sudan 14th, Kenya 19th, Uganda 20th, Tanzania 21st, and Nigeria 23rd. Poor people's crops — cassava, sweet potatoes, maize — everywhere enjoy research/output ratios well below congruence, and rich people's products, especially animal products, ratios much above; but these disparities, in particular, are substantially higher in Africa than in Asia or Latin America (*ibid.*, pp. 24–25). Despite the much greater reliance of most Asian agricultures on animal draught and integrated farming, it is in SSA that animal husbandry

Table 3. *National AR as a percentage of product value*

Region/zone	Date	Rice	Wheat	Maize	Sorghum	Cassava	Sweet potatoes	Potatoes (white)	Field beans*	Chick peas
1. All LDCs	1971–72†	0.26	0.65	0.75	0.77	0.07	0.09	0.68	0.25	0.18
2. Africa	1972–79	1.05	1.30	0.44	—	0.09	0.19	0.43	1.65	—
3. Asia	1972–79	0.21	0.32	0.21	—	0.06	0.08	0.19	0.08	—
4. Latin America	1972–79	0.41	1.04	0.18		0.19	0.19	0.43	0.60	

Region/zone	Date	Soybeans	Cotton	Coffee	Cocoa	Sugar	Vegetables	Beef	Pork	Poultry	All
1. All LDCs	1971–72†	—	—	—	—	0.50	—	—	—	—	0.88
2. Africa	1972–79	23.59	0.23	3.12	1.57	1.06	1.13	1.82	2.56	1.99	—
3. Asia	1972–79	2.33	0.17	1.25	14.17	0.13	0.41	0.65	0.39	0.32	—
4. Latin America	1972–79	0.68	0.23		1.57	0.48	1.13	0.67	0.60	1.12	—

Sources: Row 1 and fn †: York, Miller, Dalrymple *et al.* (1977), pp. 51–52. Rows 2–4: Judd, Boyce and Evenson (1983), pp. 24–25.
*Dry beans in Row 1.
†1972 for LDC output, 1971 for research outlay by national centers. If we add research by CG centres (1976 data, reduced by 30% to allow very roughly for inflation), some figures increase, and become: rice, 0.30%, wheat, 0.70%; maize, 0.81%; sorghum, 0.81%; cassava, 0.09%; sweet potatoes, 0.10%; white potatoes, 0.80%; dry beans, 0.32%; chick peas, 0.23%; cattle, 0.91%.

enjoys higher shares of AR (Oram and Bindlish, 1981, p. 54).[18]

The bias in national AR towards export crops (Pinstrup-Andersen, 1982, p. 64) and rich people's products, and away from congruence, is especially strong in SSA. Surely, the reasons are political (rather than the results of "rational" attempts by a neutral State to maximize the yield on research revenues); but the politics are those not only of conventional State machines, but also of research. Scientists like to work on projects that appear interesting, internationally respected, paradigmatic and fashionable. Research directors structure incentives, advice, and moral suasion to advance such projects. This shows up in the marked differences in ratios of AR to agricultural output among the 22 Third World geoclimatic sub-regions (Judd *et al.*, 1983, p. 20). In 1980, semi-arid tropical, cool sub-tropical desert, sub-tropical mediterranean, and semi-hot semi-tropical areas enjoyed ratios well above 1%; monsoon sub-tropical, cool-winter hot tropical, and medium terra fria tropical highland areas received below 0.55%. The location of aid support for national *and international* AR, especially sub-stations, could do much to achieve greater congruence in SSA by the use of matching grants, to help significant and well-led "critical masses" of indigenous scientists to train for, and specialize in, research into the less glamorous crops and sub-regions.

(b) *Foreign bias to African AR?*

The case for a matching-grant approach to foreign support of national AR in SSA is simple. Such AR is in most countries a foreign implantation. This at least triples the cost of a scientist-year, when the various housing, settlement, tax and other allowances to overseas experts are allowed for. Even more serious is the indirect effect in reducing the commitment of national scientists, as they are denied national leadership roles. Such national AR scientists are induced to join the brain drain out of research, often to life-long work in "Northern" international institutions or firms. Such an organization, Janus-like, publicly bemoans the shortage of SSA scientists for national AR work; and, less publicly, at once poaches those scientists for work outside Africa, and displaces them in their home countries with its own personnel.

The process is hard to stop, because there are so many private gainers from the alienation and frustration of indigenous research. Both African and Western researchers gain cash and status. Western firms and institutes — even sometimes UN specialized agencies — generate demand for their own services and a case for "aid" support from their own governments and universities. As for African governments and universities, the way for a department to gain prestige and cash is to initiate new research projects dependent on fresh foreign skills — "to grow a cabbage, call an expert from the FAO" — rather than to build on, let alone (as is often required) to rescue, old projects due for "indigenization" of leadership posts; and much rather than to do applied research that seeks out (let alone learns from, experiments with, or builds on) the decisions and environments of local farmers already successfully growing cabbages, or, more likely, local vegetables unresearched in European or US research institutions.[19]

Are these words too harsh? African food production will not respond significantly to current price-policy fashions, nor to tomorrow's fashions either, without seed-water-fertilizer-based research breakthroughs, tested for safety and profitability in smallholder environments. Given "twenty years largely wasted" in the post-Independence agricultures of many (not all) SSA countries, such breakthroughs probably require foreign involvement. But throwing money and foreign experts — usually committed and able experts, but occasionally export-reject experts — at half-analyzed research issues, to create overlapping and (in all senses) foreign research systems, will not achieve such research breakthroughs.

Look again at two rather successful agricultural systems in the SSA context. In Kenya, "out of a total of 390 research scientists engaged, less than 15% have post-graduate or research-oriented training and qualifications that would fit them into the research and development functions to be performed." In 1978, at PhD level, there were 15 Kenyans in AR — and 27 non-Kenyans (Taylor *et al.*, 1981, p. 78, 129). In Malawi, of 75 researchers, "only four (excluding expatriates) have PhD degrees," and a further 21 "have sufficient training and experience to make them effective researchers if other essential resources were available"; only three of the 10 PhDs in the Ministry of Agriculture research system in April 1982 were Malawian (Gilbert *et al.*, 1982, pp. x, 21, 46). Apart from Kenya's turnover problem, in Malawi too "promotion opportunities . . . are insufficient to motivate researchers to stay in research" (*ibid.*, p. x).

The problem in Francophone SSA countries is much more serious. *Cogestion* blends most of their research systems into dominant French institutions, methods, and even ministerial control. A multiplicity of cross-cutting, foreign-led research operations (plus, usually, a parallel but lower-status domestic operation) produces — even with complete goodwill and commitment — a mixture of neo-colonialism and anarchy. Let us look at expert judgments before numbers, and again start with a relatively successful agricultural system, that of the Ivory Coast:

> L'ensemble des institutions qui pratiquent des activités de recherche agronomique . . . constitue un puzzle compliqué et imparfait, fruit de l'histoire du pays et d'initiatives plus ou moins opportunes. Un puzzle compliqué par le nombre et la nature des institutions concernées . . . Un puzzle imparfait car toutes ces activités de recherche agronomique ne sont pas toujours complémentaires . . . Les [institutions] les plus importantes . . . — plus de 80% des chercheurs ivoiriens [sont des] expatriés — ont été héritées de la colonisation et témoignent par leur fonctionnement, leurs ressources humaines, financières et matérielles à l'omniprésence de la coopération française . . . l'ORSTOM et les huit instituts GERDAT ont leur 'siège' en France et disposent, chacun, d'un 'réseau' constitué de centres et de laboratoires de recherche en France (métropole et Outre-Mer) . . . [Ces] organisations ont eu jusqu'à présent des stratégies autonomes de coopération et de recherche, définies par leurs instances de direction (où sont représentés les Ministères françaises concernés) . . . Les implantations en Côte d'Ivoire . . . ont gagné, par la volonté ivoirienne, en autonomie par rapport aux stratégies scientifiques transnationales élaborées par Paris. (von der Osten *et al.*, 1982, pp. 15–16)

Gain in autonomy is worth having, but why should a nation, with complex agricultural problems (and deteriorating performance), sacrifice *anything* of its research policy to "stratégies scientifiques transnationales élaborées par Paris" (or London, Washington, Moscow, etc.)? Is not the result, in conjunction with the need to develop national AR institutions alongside the French-dominated ones — and the likelihood that the national institutions will offer lower pay and status — sure to be unsatisfactory?

> La balkanisation actuelle de la recherche agronomique en un nombre élevé d'institutions autonomes, de nationalités, statuts, tutelles et modes de financement différents pourrait *a priori* se traduire par la coéxistence de programmes individuels de recherche assurant une couverture quelque peu anarchique des besoins [et] l'impossibilité de maîtriser l'ensemble des institutions. (*ibid.*, p. 20)

The ISNAR team suggests that the above chaos has been largely ended, since 1971, by the activity of the *Ministère de la Recherche Scientifique*; but the team's own description of the outcomes (pp. 15–16) does not confirm that optimistic view. Conflict, confusion, and lack of Ivoirien self-reliance are inherent in the system of research *cogestion*, ultimately dependent on "stratégies . . . élaborées par Paris." The consequences go far beyond (*ibid.*, p. 44) "les multiples sinon excessives sollicitations extérieures (réunions, déplacements, visites)" — symptomatic, and contributory to brain drain, as these doubtless are.

The resulting numbers, indeed, are staggering for a country independent for almost a quarter century, and with one of SSA's better living standards, literacy rates, and — until about 1975 — agricultural records. In 1981, only 61 of 238 researchers — 31 of 178 in the "instituts cogérés," 30 of 34 in the relatively tiny national system — were Ivoirien. Foreign dominance interlocked with high emphasis on research into export and industrial crops; of the 168 commodity-assignable workers, these crops commanded 91, of whom 82% were foreign, as against 54 for food-producing agriculture (68%: *ibid.*, p. 18, 26). This outward-looking research structure is cause, much more than effect, of "la qualité peu satisfaisante de la formation supérieure [pour] la recherche économique" and of "les efforts de formation . . . désequilibrés et trop excentrés vers l'étranger" such that most Ivoriens receiving *boursier* training (in France) return to GERDAT or ORSTOM, i.e. to ultimately Paris-directed institutions, if they go back to the Ivory Coast at all (*ibid.*, p. 47). Financially, too, this is a foreign program; in 1981, the Ivory Coast paid for only 38.8% of "its" public-sector AR (*ibid.*, p. 49).

The situation is probably even worse in similarly French-dominated AR systems in really poor — and much less literate, yet more agriculture-dependent — countries of SSA. Burkina Faso is an interesting exception, in that 64 of its 123 AR scientists in 1982–83 were nationals; but this could be achieved only by incurring a cost per scientific person-year about double the levels (already exceptionally high: see Table 2) prevailing in West Africa, and — perhaps partly for that reason — by meeting "plus de 90% de l'ensemble des dépenses, même celles comprenant des allocations en paiement du personnel de la Fonction Publique . . . par des sources de financement en provenance de l'étranger" (Antoine *et al.*, 1983, p. 33). To permit significant indigenization of research *personnel* — in a system where foreign institutions at once attract brain drain and repel those seeking promotion to genuine leadership and control of national research — a very poor country must offer salaries implying *financial* alienation: Catch-22.

Burkina Faso further illustrates the dangers of diffusing research among numerous aid projects. These involved over 340 agriculture-linked

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missions in 1973–82. These mostly generated projects with 1–2% of supporting research (*ibid.*, p. 33):

> La prolifération du nombre et des sources de financement, la compétition et le double emploi, les objectifs différents des projets semblables, etc., [mènent à] l'accroissement incontrôlé des aides extérieures[, et] la manque de coördination entre elles [soulève] des problèmes extrêmement graves, en particulier au niveau de la capacité d'absorption des aides.

It is in this context that we must assess SSA's relative, and extreme, failure — by LDC standards — to achieve "establishments of about 100 [indigenous, properly qualified AR] scientists . . . which would permit three national commodity programs to be operated, each with a central station and three to four substations" (Oram and Bindlish, 1981, p. 31). It is not a question of smallness of countries alone: Nepal, Papua New Guinea, and Jamaica — but in SSA only Nigeria, Kenya, Sudan, Ghana, Tanzania, Zambia, Cameroon, Ethiopia, Malawi and Zimbabwe — have reached this level of establishment. Even among these, most feature low indigenization and general under-qualification (notably lack of PhDs).

Many SSA governments rightly wish to avoid post-haste indigenization at the expense of research quality. However, foreign-dominated research systems — where expatriates come and go and bring prestige, cash, and draining prospects for national brains — have at once severe developmental limitations, and built-in pressures: to maintain themselves; to research for Western journals, often at the cost of local relevance; to insert Western colleagues; and to export, rather than to promote, local talent. Given the long-term problems of most SSA educational systems, the only feasible solution to the immediate dilemma — that rapid indigenization loses quality, but that slow indigenization is infeasible given the above pressures — is to develop agricultural policy and AR systems that are less costly, in general and especially in terms of expatriate skill and money. We return to this issue in Section 6.

(e) *Socioeconomics and agricultural research design*

One much-discussed way to increase research relevance, and to save on imported experts, is a greater "socioeconomic" orientation of AR. This needs to go well beyond a shift from commodity research to farming systems research. FSR, while surely needed to bring researchers closer to farmers — and to learn about underresearched, especially humid, farming — has been "sold" in ways that arouse[d] excessive expectations. Economics and soc[iolo]gy, in respect of their relevance to design and implementation of output-increasing and/or poverty-reducing agricultural research, neither begin nor end with FSR.

Economics and other social sciences are most needed when the natural scientist appears most confident that they are not: that he or she can dispense with their confusions, and can do a simple piece of cost-accounting, or an arithmetical breakdown of causes of the "yield gap" (between farmer and experiment station) into, say, lower levels of fertilizer, pesticides and planting-date practices. Prices, factor availability, marketing costs, taste preferences, and social factors — including rules about how cash from farming is distributed, among agents and within households — are critically important in explaining a "yield gap." The fact that maize hybrids with improved practices in Kenya "show potential of 10t/ha and an actual average of 2t/ha" (Taylor *et al.*, 1981, p. 31) may well indicate that research stations are making big losses to maximize levels of management and inputs, while smallholders are rationally seeking a preferred combination of profitability and security. It is too easy for AR scientists to propose — and for visiting experts to confirm — that, for example, "abandoned tea fields suggest that the new smallholders do not yet fully understand the risks and opportunities of intensive tea cultivation" (*ibid.*, p. 36). A microeconomist would first test the possibility that the intensive advice might conflict with smallholders' preferences or even feasible options; a planning economist, the possibility that major switches to tea production (given its effect on prices, plus likely exogenous price trends) might not be desirable for Kenya, even if such switches *were* profitable for "the new smallholders."

Increasing labor/land ratios raise both the need for, and the difficulty of, fairly sophisticated socioeconomic research inputs into policy. Such rising ratios usually cause fragmentation of holdings (of any given total size) into several small plots, as parents seek to endow children with "fair shares" of different sorts of land, and as farmers acquire or lose parts of plots through resumption or foreclosure. It is normal form for outsiders (including first-class scientists) to see fragmentation — *morcellement* — as a plain evil, to be ended by legislation (an unusually sensible discussion is Contant *et al.*, 1982, p. 9). In reality, fragmented plots — especially where a rising labor/land ratio provides plenty of time to move among them — can be a sensible way to spread risks. Consolidation is costly, and arouses suspicions, though some Indian States have succeeded with it.

Thus AR scientists, in general, should try to provide recommendations that are safe and profitable in the context of prevailing, or likely future, smallholder practice — in regard to plot size, crop mixture,[20] or anything else — unless the recommendations are so profitable or otherwise attractive that they will break down existing practice.[21] Neither of the gut reactions of many agricultural scientists, *viz.* to blame the "irrational" farmer or to seek to change his practice without changing the conditions that largely determine it, is likely to help (cf. the decades of useless preaching about composting, pure line stands, early planting, etc.). Economists and social scientists can help, by establishing not just what farm systems are, but why different times, places, and "relations of production" (both production-function and inter-class) induce different behavior, among various sorts of smallholder, in response to proffered new technology (an excellent example is Pingali *et al.*, 1987). It is partly because sociology and economics are needed early in research design — and because general sociologists and economists, macro and micro, need to learn from (and sometimes to teach) AR experimental scientists — that an FSR perspective, while often necessary, is very seldom sufficient to guide AR policy. The intellectual history of India's green revolution confirms Collinson's East African observations (1982, p. 9) that

> Historically economists had carried out *ex post* studies of agricultural technology — "coming back from the field" to inform biologists that they had "got it wrong." Such an approach was not only unconstructive but built up antipathy in research establishments towards these "commentators."

For reasons already suggested, earlier socioeconomic involvement has macroeconomic and other analytic requirements, going beyond FSR.[22]

(d) *Scope and limits of farm systems research*

FSR insights, as *part* of a socioeconomic input, and if not unduly dependent on rapid rural appraisal techniques,[23] have great potential value in AR design. This is especially true in environments where very little is known about what small farmers do — let alone why — and where, therefore, projects have too often been imposed that (while almost certain to fail) cannot be evaluated at any stage: tropical rainforest farming environments, notably in Zaire, are a clear case. But how are FSR insights to be acquired and used?

One way is through a two-team approach, with Commodity Research Teams as well as Adaptive Research Teams. This was adopted in Zambia by CIMMYT, in response to earlier conflicts between biological and FSR approaches within the Kenyan program (Collinson, 1982, pp. 32, 34). It has its own problems, and is costly of scarce experts. However, the two-team approach avoids the serious risk that a wholesale switch to FSR might undermine commodity research, which is at least the locus of such past successes as AR has achieved in SSA (Anthony *et al.*, 1979, p. 252).

FSR addresses a real problem: that "disciplinary and commodity-based research institutions are often not conducive to farm-oriented experimentation" (Collinson, 1982, p. 8). Hence, for example, advice to plant maize early "addresses a symptom and not the problem" that oxen are too weak to plough until the main rains increase pasturage (*ibid.*, p. 15). However, FSR, apart from its incompleteness as (and propensities to claim sovereignty over) agro-rural socioeconomics, has major internal problems. First, the farm's system tends to be improperly isolated from the social system to which it belongs. Collinson, a skilful and subtle FSR practitioner, would handle the above "problem" — that "the farmer" has too little pasturage for early ploughing and hence for early planting — *inter alia* by "(a) Improv[ing] the feed supply in the dry season" and also by " (b) (i) providing artificial insemination services which reduce the need to carry male animals; (ii) opening up market opportunities for male calves; (iii) encouraging use of cows for draught purposes." All these measures, and several others suggested by Collinson (*ibid.*, pp. 15–16, 39), would increase returns to private herd ownership. Could this raise private herd sizes and hence pressure to increase stocking rates[24] upon common pasture, degrading it and inducing the "tragedy of the commons"? Of course, Collinson fully grasps this point; but the perspective of the *farm-by-farm* system can provide blinkers, excluding the consideration of aspects of the *social* system that may turn private optimality into social unreason.

Second, the single-farm system also excludes aspects of the family economy. Work in SSA and elsewhere suggests that about a quarter of rural working time, and about a third of income, is typically non-agricultural (Chuta and Liedholm, 1979). Moreover, as land scarcity grows, so, probably, will the proportion of agricultural work that rural people perform on other people's

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farms. Here, *farm-by-farm* FSR excludes constraints, trade-offs and options setting on-farm against *off-farm* activities.[25]

Third, FSR can be unduly conservative. It tends to be based on survey accounts of recent experiences -- perhaps based on the interviewees' past norms. But these experiences and norms reflect a peasantry less differentiated, less land-scarce, and less exposed to innovations and delivery systems than the farmers who will be approached with the packages produced and field-tested, perhaps 10 years later, by the AR scientists whom the FSR has guided.

FSR's great strength, and the reason why it must be a *part* of the socioeconomic input into AR, is that it emphasizes on-farm adaptive research (OAR). OAR is indeed essential. Rwanda typifies much of SSA in that "les innovations techniques proposées par la vulgarisation ne passent pas dans les systèmes de production traditionnels [à cause de] l'évidente inadaptation du message de la recherche aux besoins et à la situation socio-économique des cultivateurs" (Contant *et al.*, 1982, p 67). However, to equate OAR and FSR is surely a mistake (a very useful discussion is Simmonds, 1985). For example, Sri Lanka's extension service, since the late 1950s, has helped selected progressive (not big) farmers to choose among alternatives by testing fertilizer-varietal combinations in a tiny on-farm Latin square, typically 3×3. That is excellent, action-oriented OAR, but it is not FSR.[26]

Certainly, no AR will reach and persuade farmers unless its results are proven sufficiently profitable and safe at farm level. Currently, AR systems in SSA seldom reward researchers either for proving to farmers the validity of their advice, or for successfully issuing proven materials or advice to extension services, nearly as well as for producing PhDs or journal articles. Socioeconomic inputs should include OAR — not only FSR — "designed into" general AR. But much more is required of such inputs.

5. AGRICULTURAL POLICY: NEEDED BEFORE RESEARCH POLICY?

(a) *The crucial issue of statistics and information*

Priorities must be guided by an overall agricultural policy framework. This requires reasonably timely information, with confidence intervals (95% or at worst 90%) for numbers no more than ±10–15% around best estimates for main farm outputs, inputs, and prices — and for calorie deficiencies in vulnerable groups — nationally and by major agroclimatic zones.

Such data, and the associated information systems (including trained workers to collect and process data) are created by a small subset of AR scientists (plus rather more statistical technicians), but these data are essential inputs for sensible design of all national AR. If we are not even 90% certain that, say, millet output in a typical year lies between X and 1.5X tons in a given agroclimatic zone, and/or that area planted lies between Y and 1.5Y hectares, then how can anyone competently set regional priorities for yield-increasing research?

Unfortunately, that degree of ignorance is normal. Barely half-a-dozen SSA countries have reasonably reliable data on smallholder food outputs. National statistical cadres are sparse, and their status is low. Numbers are often not used at top policy level because they are known to be randomly and substantially unreliable. Then, because such numbers are known not to be used, senior officials place little priority on improving reliability. Livestock numbers, yields, and *a fortiori* management practices, in traditional herds are even less understood.

Pan-African data, and most national data, purporting to show output trends — though reproduced in reviews of research policy — are worthless (Taylor, 1981, p 26, on maize yield trends in Kenya; Antoine *et al.*, 1983, p. 14, for Upper Volta, and pp. 10–13 for strong inferences from these numbers; von der Osten *et al* 1982, pp. 6 and 8, for the Ivory Coast). A review for Malawi states frankly that, for smallholders, "detailed information on cultivated areas and production or yields has not been available except for a few small areas," yet claims that smallholders' "maize yields are stable at about 1,000 kg/ha under average weather conditions" (Gilbert *et al.*, 1982, p. 5). Indirect inferences, e.g., that in most of SSA food production is going badly, are feasible from prices, nutrition surveys, and international trade data. But, without direct and regular information about aggregate and zonal levels and trends in main crop yields and areas, agricultural policy — including policy design — is at best intelligent guesswork. How can one know which crops do well, where, when, under what weather or input-delivery conditions, if one does not know output levels at all? Better agricultural and food statistics are in most of SSA a necessary, and inexpensive, precondition for significant policy improvement, and therefore for AR design based on improved policy or feeding into it. The data problem will not be solved by a once-for-all fact-finding or sample-survey exercise, overfunded from abroad and based on non-replicable computer high-tech. Needed are indigenous statistical cadres, using standard procedures, rewarded by sound career structures, delivering timely data with known levels of vulnerability to error for major crop areas and yields, and feeding such data into a politico-bureaucratic system that can use them. Such things are taken for granted in much of the Third World, including some countries in SSA — but, as yet, too few.

(b) *Why is policy needed? Transformed land/labor ratio*

Why bother with a statistical framework, or indeed an agricultural or AR policy? Why shouldn't SSA governments, overstretched as they are, simply stop depressing incentives to farmers, and then leave farmers to signal their own research requirements? ISNAR's review of Rwanda excellently documents the problem with this approach. Traditional systems, near-optimal when arable land was ample, cannot readily switch to yield enhancement as land is exhausted. Incentives, inputs, public investments, and technologies produced by the AR system require to be readjusted by policy, if old (or new) systems are to cope with rising person/land ratios:

> La société rurale traditionelle a ainsi réussi . . . à faire évoluer et maintenir l'efficience de son système de production . . . mais les défis du présent ne sont pas les défis du passé: il ne s'agit plus aujourd'hui de réaliser l'occupation d'un nouvel espace agricole, mais d'intensifier l'agriculture traditionelle dans une situation où les limites des terres cultivables sont très près d'être atteintes. Or, il apparait que ces mêmes institutions de la société rurale, qui ont favorisé les adaptations passées, tendent à jouer aujourd'hui dans un sens totalement dysfonctionnel pour le développement agricole. (Contant *et al.*, 1982, pp. 9–10)

Rwanda is an extreme case of confrontation between rapidly-growing rural populations, slow or capital-intensive off-farm growth, and near exhaustion (or rapidly-rising marginal costs) of prospects for creating extra livelihoods with existing techniques on arable land. However, most of SSA increasingly presents this confrontation, and will do so more sharply when the results of any current changes in AR policy reach farmers. As in Rwanda, much more active agricultural policy — to provide direction for public-sector research generation and diffusion — will be required. In Rwanda, agricultural education — and housing — also require adaptation to rising person/land ratios; the problem transcends that of research policy (*ibid.*, pp. 9, 67). In the Ivory Coast, too, higher education for agriculture requires new structures, and research requires new sectoral priorities, in view of "l'intensification inéluctable de l'agriculture" (von der Osten *et al.*, 1982, p. 57). Kenya, with 4% yearly population growth and a growing class of recognizably landless farm laborers, presents a similar case.

Rapidly rising person/land ratios, normally accompanied by non-farm labor absorption and sharp increases in marginal cost of making land usable for agriculture, have major implications *via agricultural policy* for AR. It may (Hayami and Ruttan, 1971, 1985) adapt in the medium term to factor intensities, so that a more labor-intensive innovation stream will ultimately be produced, even by public-sector systems, as a result of changing on-farm scarcities. However, this takes time — research projects, and even research design decisions, now in the SSA pipeline, reflect today's or even yesterday's land/labor ratios, not tomorrow's dramatic switches to intensive farming. Also, there is a supply side of (i) "labor-saving" AR and (ii) "land-saving" AR. Not only basic and first-order-applied science, but also the pressures of exceptional big farmers (normally relatively extensive) and of researchers' career priorities and even fashions, *locate* the supply curves. Their *slopes* indeed respond to "prices," so that (ii) increases relative to (i) (derived demand) as rising labor/land ratios shift on-farm requirements; but the responsiveness may be small, the *location* of the supply curves not sufficiently labor-intensive, and adjustment slow. What is required for agricultural, and AR, policy to speed it up?

The effects of current and recent population growth largely determine SSA's labor supply for the next two decades. Sharply growing labor intensity, in an environment of increasingly scarce land and slow off-farm labor absorption, is therefore a medium-term certainty. This is alarming mainly because, in a region where undernutrition is already a major problem, it tends to depress further both the real wage rate (and the real return to labor time in farm self-employment) and the proportion of time spent in productive work — and, therefore, per-worker purchasing power over food and/or capacity to grow it on-farm.

The main responses of AR to this, so far, have been outside SSA, where land scarcity has been clearer, and have involved AR-based seed-fertilizer strategies, mostly based on greater water reliability. The question of how much SSA can or should, in AR and underlying farm policy, "learn from other LDCs" is not treated in detail here (see, however, Lipton, 1985). Clearly there are negative as well as positive lessons. If modern varieties create seasonal labor shortages, it is normally better to subsidize migrant laborers rather than tractors (or fuel or credit for tractors). Worsening distributions of land and capital can and should be averted — the small family farmer's advantages, in conditions of high person/land ratios, are not reduced by the seed-based technology. SSA, most of it initially with more (and more equally distributed) farmland per person than India or northern Mexico, could be better placed to avoid these second-generation problems, so that extra food meets extra purchasing power among hungry workers, and does not merely displace imports.

(c) *Essentials of agricultural policy to guide researchers*

What AR, and supporting agricultural, strategies would improve SSA's prospects of being able to afford the luxury of worrying about second-generation problems? The detailed needs are of course highly specific to agroclimatic zones and factor scarcities (Mellor *et al.*, 1987), but an overall statement is possible. AR requires (1) a strategy for *food* and (2) *water*, with reliable and timely (3) *information* and backed by a massive (4) *transfer* of domestic SSA resources from other sectors into agricultural development. This should be used to bring (5) *field-tested*, reasonably safe (and hence (6) *water-controlled*), (7) *fertilizer*-supported, and increasingly (8) *intensively-farmed* (9) *high-yielding* varieties of (10) *major*, (11) *currently-grown* (12) *cereals* and *root* crops to (13) *smallholders*. This may sound dull; in fact it is highly controversial.

(1) *Food strategies:* Several SSA countries have stated these and a few have begun to implement them (Heald and Lipton, 1984). However, there is considerable confusion about goals — should they be food security, import-saving, efficient farm growth, or (often largely neglected) adequate nutrition? There is undue emphasis on price policy, even where lack of spare land, of improved technology, or of input delivery capacity means very low medium-run price-elasticities of total agricultural supply. The African hunger of 1983–85 appeared, in some of SSA, to be producing the same sort of dismantling of shibboleths as did the Indian hunger of 1965–66; but, in many countries, the anti-rural biases of conventional politics have reasserted themselves. Donors, even the European Economic Community (EEC), have shown too little patience in gearing aid around, and raising it in support of, implementable food strategies, sensibly confined to a small number of countries. The lack of congruence discussed in Section 3 will have to give way to much greater AR stress on smallholder food production, probably, at least for some time, at the expense of research into some export crops.

(2) *Water strategies:* For most of SSA, seed-fertilizer technology is the only thing on the horizon that can produce the big increases needed in food output per person. Many improved seeds do better than older varieties at *any* level of water input that permits output at all, but most achieve *considerable* increases in yield (and profitability) only in reasonably good water conditions.

In some of SSA, floodwater and/or rains are reasonably adequate in quantity in, say, eight or nine out of 10 main seasons. AR strategy then needs to concentrate on screening and field-testing (a) main-season varieties able to defer crucial stages in plant growth if moisture is inadequate; (b) second-season varieties efficient in using residual soil moisture.

In most of SSA, however, major rainfall uncertainties affect not just food security, but the willingness of poor smallholders in imperfect credit markets to accept the risks associated with seed-fertilizer innovation. AR in such areas has stressed the adaptation of farm systems (planting dates, manuring, etc.) to possible water shortage, usually with scanty results. But this stress is due to past misconceptions (based on past land-plenty!) about the unviability of irrigation. If it is *assumed* that water supply cannot economically be adapted to farming systems, then smallholders, or researchers, must adapt systems to water inadequacies. Increasingly, however, the assumption is wrong.

Unfortunately, capacity to adapt research to a new intensive water strategy is weak. In Kenya's "lower rainfall areas, the mission was surprised by the almost total lack of research expertise or scientific thrust in the fields of agroclimatology or water-resource management" (Taylor *et al.*, 1981, p. 47).

We return to irrigation options under (6) below. Here, the point is that for most of SSA, food strategies imply seed-fertilizer strategies, which in turn imply water strategies, for which new AR priorities are needed.

(3) The *information* issues were dealt with above. AR directors and planners require, from policymakers, timely information about levels and trends in key input and output variables. What makes yields differ — across regions, farm types, etc.? Where are yields low, or fluctuating, for reasons that research might cost effectively remedy? Of course, a big discovery, like IR–8 rice, or SR–52 hybrid maize, can in appropriate areas triumph even in a statistical near-vacuum. But research resources cannot be allocated by

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assuming such discoveries. Normally, effective allocation requires some key numbers.

(4) *Resource transfer:* Although disparities between farm and farm and non-farm income per person typically range from 1:5 to 1:7 in SSA, as compared with a norm of 1:3 in Asia (Lipton, 1977, Table 5.4) — and although shares of population and workforce dependent mainly on agriculture are generally higher in SSA than in Asia — the proportions of "government expenditure on economic services" that go to agriculture are typically somewhat lower in SSA than in Asia (IMF, 1983, pp. 43–45 and country pages; cf. especially p. 366 and p. 580 to allow for federal aspects in South Asia). Although few data are available for capital account, it seems likely that the generally low levels of irrigation outlay in SSA have kept shares of public investment going to agriculture well below South Asian levels. Proportions of *total* investment going into agriculture, irrigation, forestry and fisheries — for whatever the data may be worth — are amazingly small in most of SSA compared to proportion of workforce. Agriculture and allied sectors receive only 1 to 3% of gross fixed investment in Botswana (with about 80% of workers dependent mainly on the sectors), Burundi (over 80% of workers in the sectors), Togo (about 70%) and Zambia (about 65%). About 6 to 8% of total investment goes to agriculture, etc., in Kenya (which has about 75% of workforce in the sectors), Tanzania (80%) and Mauritius (28%); 12% in Lesotho (82%), and Zimbabwe (57%); and 16% in Rwanda (almost 90%) (FAO, 1983; UN, 1983). In most of SSA, smallholder agriculture is starved of every definable resource allocated by government, from fertilizer to skilled administrators and Cabinet time. This is not an environment in which research outputs can elicit the complementarities required for a good response.

(5) The balance between research-station experiment and *field testing* might seem a purely research issue, but it is part of the political context of agricultural policy — whether farmers are to be told what to do, what crops to grow (as often in Tanzania, in Sudan's Gezira and nearby schemes, in Zaire, and elsewhere), etc., or whether their feedback is to be listened to by AR scientists. "Relevance, except as a 'good thing,' is still a hazy concept; a prescriptive mentality often still dominates technical research work" (Collinson, 1982, p. 10) — natural enough if, in their hearts, the researchers' ultimate employers, senior politicians and civil servants, see smallholders as ignorant rustics in need of a push from outside. Can the whole agricultural input and delivery system instead be given career incentives dependent on the progress of the farmers that it serves? That is easier said than done; but few SSA public sectors are trying very hard. ISNAR documents, in all its reports cited here, the absence or irrelevance of schemes-of-service in research and extension. Few, if any, persevering attempts have been made to see that public-sector personnel (or agencies) for rural SSA are rewarded *pari passu*, not with age or experience, exams or publications, but with measured contribution to rural output or welfare. In such circumstances, "field testing" is unlikely to catch on. It is a policy issue.

(6) *Water control:* SSA in 1981 irrigated about 3.5m of its (roughly) 130m arable hectares, or 2.7%. "Developing" Asia (excluding Japan and Israel) irrigated about 131m out of 424m arable hectares, or 30.8% (FAO, 1983, Tables 1 and 2). Soils and scarcities do not explain much of this; they differ more within continents than between them. SSA's low degree of irrigation reflects *past* facts — land plenty, low levels of public sector productive involvement, few fertilizers or high-yielding varieties — more than *present* desiderata. Who can expect enthusiasm for AR, agricultural investment or farm policy — either from farmers or from governments — (a) on the base, in a normal year, of the low yields typical of very badly watered holdings, let alone (b) during a drought, in which the returns to past agricultural investments are slashed due to lack of irrigation?

Almost all farmers are risk-averse, but often only moderately so (Binswanger, 1981). Some fluctuation in returns often seems acceptable to potential adopters, even poor unirrigated smallholders (Smith *et al.*, 1983). But the extreme uncertainties of much of non-equatorial SSA are something else again. Fertilizer or ox purchase can well show a zero return for two or three years running. Some increase in water security, at least in semi-arid areas, seems indispensable for faster offtake of given seed-fertilizer AR. Yet in the Sahel less than 1% of cultivated lands were irrigated; in 1970–79 "new surface area under irrigation barely exceeded [area under] old installations becoming inoperable" (Fell, 1983, p. 113).

The discredit in which much irrigation stands in SSA is based on big, ill-planned schemes that often deteriorated sharply when donor overview ended, and that almost always required complex coordination and delivery. Both AR and policy need to shift towards on-farm, farmer-controlled micro-irrigation — from sandrivers, other groundwater, surface lift, and sometimes even rainwater catchment. Permeability of soils sometimes explains the reluctance of SSA smallholders to irrigate, even from nearby surface

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water (where their Asian or North African counterparts would set up animal-drawn or *shadouf* lift systems at low cost). However, there is more talk than knowledge about the local soil conditions, the cost of raising and distributing water (*not* by sprinkler systems that assume cheap capital), the erosion and run-off effects, and (in the case of groundwater) the depth of table, distance to aquifer, and recharge rate.

A few big, badly managed gravity-flow systems must not be an endless excuse for writing off even micro-irrigation in SSA. Certainly, rehabilitation deserves priority, and big river-basin schemes skepticism. However, I question the view of Oram *et al.* (1979) (see also FAO, 1986) that irrigation must continue to contribute negligibly to SSA food output. Worldwide, despite major problems, "all but eight [of 40 World Bank irrigation projects reviewed] had audited economic rates of return (ERRs) of 10% or better; more than half exceeded 15% . . . 19 exc[illegible]ed their projected ERRs." Even by 1977–8[illegible] [illegible]5m, or 9% of world aid to irrigation, w[illegible] SSA (Carruthers, 1983, pp. 31, 39, 139). Without large public outlays, first to investigate water resources and then (where economic) to develop them, it is unlikely that AR can generate the requisite growth. Neither watershed management, nor attempts to breed high-yielding varieties resistant to severe moisture stress, after 15 years of hard work at ICRISAT, IRRI and elsewhere, seem promising enough to substitute for water-supply control. Can the political will to finance agriculture be mustered if agriculture is so often wiped out by drought? SSA irrigation experience indicates that "agronomy is as important as hydraulics" (Fell, 1983, p. 114); they need integration in research, as well as in irrigation design and maintenance.

(7) *More fertilization*: Recently released varieties of major cereals, given reasonable water and light conditions, outyield traditional varieties even at low or zero N and P fertilizer supplementation on the great majority of non-desert soils. But a really substantial addition to yield is obtainable, except in very rich soils, only with some chemical fertilizer. Organic manure can complement this, but there is seldom enough, near to the crop, to substitute significantly for inorganics.

Fertilizer policy in SSA faces two myths and two real problems. The myths are the "package" and the "standard mix." The real problems are formerly low returns to fertilizer, and high delivery costs.

The ideal of a set package, determined from the research station and imposing on numerous different soils (and rotations) the same NPK recommendations for any given variety (Pinstrup-Andersen, 1982, pp. 52–53), makes less sense than the Sri Lankan on-farm experiments in which each farmer can learn, from his or her own conditions and experiences, what extra nutrients maximize the desired profit-security combination.

Both package and on-farm approaches, however, require that farmers can choose among various fertilizers offering, for example, N without unwanted P. In much of SSA, though almost nowhere in Soutn Asia, farmers are confronted — if the fertilizer arrives at all — with a take-it-or-leave-it NPK combination termed, say, "D compound." Except by chance, the combination on offer will be sub-optimal, and some of the nutrients bought will be wasted. This plainly deters fertilizer purchase, especially if credit is costly, delivery untimely, or the two ill-synchronized.

Many SSA countries have experiments, often from the 1950s or early 1960s, claiming to show that fertilizers have poor economic returns. Some of these experiments are useless, lacking data to perform tests of statistical significance — and even information about where the fields were located. Even the competent AR dates from a time when the ratio of the value of food outputs to that of fertilizer inputs was generally lower than today. Types of fertilizer and possibilities of application were less developed; and few high-yielding (i.e. in general much more fertilizer-responsive) varieties, hybrids or composites of major food crops were available. Up-to-date, on-farm AR into smallholders' economic returns to, and risks from, modern NPK inputs at different levels and combinations, and with different varieties (as well as crop mixes and rotations), is urgent in most of SSA. Where appropriate, it should be combined with work on the effects, on these returns and risks, of varying degrees of water-control and moisture-stress.

(8) *Intensive farming:* AR and agricultural policy in most of SSA need to switch emphasis from extensive to intensive farming, and thus from labor-saving large farmers to land-saving and labor-intensive smallholders. Of course, both land and labor are seasonal inputs; in principle, relieving a seasonal labor scarcity *may* permit double-cropping, and therefore absorb labor *glut* at other times of the year. However, the argument has more often been an excuse to get a tractor subsidy than a scientific observation.

AR should certainly use FSR or activity analysis to alert agronomists, etc., to seasonal issues. But a seasonal labor shortage (or, more accurately, wage-inelasticity of supply) is as likely as any other labor shortage to be eased as popu-

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lation growth raises person/land ratios, i.e. by the time the research recommendation has been field-tested and is ready for delivery to farmers. Even a genuine and persistent seasonal labor shortage can often be alleviated by other means than replacing workers by tractors or weedicides, e.g., by selecting shorter-duration or weed-shading varieties, or by subsidizing or organizing seasonal migration from areas with different peaks.

(9) What of policy towards *high-yielding varieties* (HYVs) in SSA? Worldwide, the main success stories are water-controlled rice and wheat. Both, in SSA, are currently urban-consumed crops, often grown and eaten by the less poor, and likely for the foreseeable future to be imported. With notable exceptions, wheat is not a very promising crop for tropical SSA. HYV rice is likely to be confined mainly to irrigation schemes in West Africa.

In any case, and with due allowance for the awful numbers, crop development in SSA cannot plausibly rely on rice and wheat. In 1982, according to FAO,[27] in the "market" LDCs of South and East Asia, rice comprised 56.7% of cereal output by weight, and wheat comprised 23.5%; the corresponding figures for SSA were 9.6% and 3.3%. Over 30% of SSA's rice was produced in Madagascar, and a further 44% in Guinea, the Ivory Coast, Nigeria and Sierra Leone. Of SSA's small wheat output, 46% is attributed to Ethiopia and 13% to Kenya.

Clearly, any HYV-based cereals expansion in SSA is likely to have to rely mainly on maize, sorghum and millet. Respectively, these comprised 31.1%, 23.4% and 21.4% of SSA's estimated cereals output in 1982 (i.e., over three-quarters in all), as against, respectively, 8.7%, 5.2% and 4.3% (i.e., less than one-fifth in all) in South and East Asian LDCs. Moreover, millet, sorghum and maize are widespread over most countries of SSA. Whatever allowance one may make for the possible rapid spread of rice and wheat HYVs — and wheat HYVs in Bangladesh have surprised most of us — it has to be on other "poor people's staples" that *cereal* HYV expansion in SSA will depend for many years to come.

Moreover, cereals do not loom so large in SSA's output of starchy staples as in that of the developing market economies of South and East Asia. For every kg of cereal grain produced in this Asia region, about 230g of roots and tubers (excluding potatoes) were produced, and about 60g of pulses. In sub-Saharan Africa, for every kg of cereal grain produced, the respective figures were about 1,900g and 115g. Plainly, if "plasticity of nature" can be assumed, cassava, sweet potatoes, and perhaps dry beans — as well as maize, millet and sorghum — are much higher research priorities for SSA than for the "green revolution" heartlands of India, the Philippines, etc. Also, these crops are the poorest people's staples, in both production and consumption, and thus lie at the center of the nutrition problem. (In parts of India, wheat production costs are brought so far down that wheat becomes cheaper per calorie than maize, millet or sorghum (though hardly than cassava); but in most of SSA the long-distance delivery costs, from the few places where wheat will grow — or from the ports — render this very unlikely.)

However, SSA's crop-mix is less unfavorable to HYVs than is often believed. The so-called "green revolution" is not just a wheat-and-rice yield breakthrough. From 1949–50 to 1964–65, before the HYVs, the Indian trend rate of growth of output-per-hectare was 2.2% for rice, 1.5% for sorghum, 1.3% for millet, 1.2% for maize and 1.3% for wheat. Following the droughts of 1965–66 and 1966–67, Indian yield growth resumed in the HYV era, 1967–68 to 1981–82, at 1.5% for rice, 3.2% for sorghum, 0.8% (not significant at 5%) for millet, 0.4% (n.s.) for maize, and 2.6% for wheat. In the latter period, sorghum achieved very rapid rates in several states (7.0% in Gujarat, 5.4% in Maharashtra); so did millet (8.7% in Karnataka) and even maize (4.7% in Andhra) (Sawant, 1983: 491, 493). The dramatic, widespread success in Karnataka of HYV finger-millet and horse-gram (Rajpurohit, 1983) deserves special attention from AR in SSA.

Thus, in the HYV period, India's sorghum yield outpaced rice and wheat; and all three key "African" cereals — maize, millet, sorghum — did very well in some Indian states over this period (see Jansen, 1988, for recent confirming evidence). Similar data could be compiled from other Asian countries. Moreover, India's western region "demonstrates that impressive growth in yields through . . . fertilizers and HYVs is possible even under conditions of low irrigation" (Desai and Namboodiri, 1983, p. 507) and shows no evidence of deceleration, or even as yet of diminishing returns to expansion of inputs into new areas.[28] The recent spread of a new generation of hybrid maize in Kenya, and of hybrid sorghum (as well as maize) to new areas in Zimbabwe, confirms these possibilities for semi-arid areas in East Africa.

This argument, however, should not be pushed too far. (a) Millets *have* shown stagnant yields in most developing countries. (b) Cost-reducing wheat and rice HYVs have displaced "poor people's crops" on some of the lands best suited to them; hence their output has seldom grown as

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fast as yields, even in the successful states of India. (c) The successes with maize, and to some extent sorghum, have depended mainly on hybrids, which require a reliable method of issuing quality seeds, on time, each year, far-fetched in much of SSA, where composites and synthetics make more sense, but have until recently been much less researched. (d) Neither pulses nor root crops show the sorts of breakthrough associated with wheat, rice and perhaps sorghum and maize. ICRISAT claims substantial offtake (c. 10m acres?) for at least one HYV of pigeon-pea; and Indonesia shows, if the data are reliable, steady and substantial yield growth for cassava (FAO, 1983, p. 128); but the general picture is not good. (e) There is much doubt about the transferability of HYVs even between apparently comparable agroclimatic circumstances in Asia and Africa.

However — while farmers' own experiments, if supported by mutual learning with national research systems, can make some contribution (Richards, 1985) — most of SSA has no alternative to HYVs, given the food-population constellation. Currently, few national systems are adequately placed to screen, breed and test germplasm from international research centers, and to convert it into field-tested releases. Until the national cadres are available to do this, price-responsiveness alone will do little for us (Lipton, 1987).[29]

(10) *Major crops:* The data on "congruence" (Table 3) speak for themselves. Some contrasts are shocking. SSA spent in 1976 some $5.47m on national AR into soya beans — $3.75m in Nigeria alone, and $1.72m elsewhere. Comparable outlay for cassava was $2.93m, $2.69m in Nigeria and $0.24m elsewhere (Judd *et al.*, 1983, p. 70). Soybeans covered 0.22m ha of SSA in 1974–76 and 0.32m in 1982 — 0.17m and 0.19m respectively in Nigeria. Cassava covered 6.6m ha of SSA in 1974–76 and 7.6m in 1982 — 1.0m and 1.2m in Nigeria (FAO, 1983, pp. 128, 138). Nature has to be *very* unplastic for that to be right.[30]

(11) *Currently grown crops:* SSA history contains several examples of the introduction of new crops, often at smallholder level, to major regions, and with dramatic success. Cocoa in West Africa is a striking illustration. However, if the incentives, delivery systems, water and societal and technical environments to develop and select options are right, these things happen anyway. Food output and agricultural output per person in most of SSA since 1965 have probably fallen, in part because these matters are not right. If policy and AR are to produce the rapid turnaround required, they probably should not, like Sisyphus, seek to roll uphill the stone of introducing major new crops — soybeans, wheat, cotton — where they meet with peasant (or market) resistance. There are exceptions, but generally the "congruence" question is the right one.

(12) *Cereals and roots:* This apparently innocent priority hides a mass of argument. It is export crops and livestock that show high AR/output ratios in most of SSA (Table 3). Foreign advice to SSA governments, noting the tendency of marketing boards to depress prices to smallholders growing for export, concentrates on improving the price (and other) environments for export crops. Moreover, trade is heavily subsidized through roads and other marketing infrastructures, while food aid — which, while it has great virtues, must to some extent cut domestic food prices and reduce governmental priority for food output — has no counterpart in respect of non-food products. The international environments, then, are heavily biased in favor of steering farmers away from basic food products, especially given the high protection or subsidization of these products in developed countries (World Bank, 1986, Chaps. 6–7).

Most economics is permeated by a *bias towards trade*. (Listians and other protectionists seek more intra-national exchanges, "national integration and development," but the bias is the same.) French and Scots 18th-century economists correctly demonstrated the damage done by constraints on, respectively, internal and external exchanges. Their successors, whether operating with what Seers (1983, Chap. 1) elegantly terms "Marxist or other neo-classical models," have illegitimately firmed up this opposition to barriers against trade into support for artificial stimulants to trade. Although opposing overt export subsidies, standard economics today certainly supports covert stimuli to exchange: "free," i.e. publicly-financed, roads and other communications, and numerous measures to subsidize factor and product mobility. This implies, at given resource use levels, taxation of production for nearby consumption, especially for subsistence consumption. This set of policy preferences, closely linked to the urban character of policymaking, has steered national AR away from locally-consumed food products, especially those whose high weight/value ratios and/or processing costs render trade difficult. The research activities of colonial and neo-colonial research organizations confirmed this pro-trade bias, especially if they were linked to firms that (a) had some market power and (b) processed (as well as growing) such products as cocoa or palm oil. Such firms can internalize some of the benefits, from cost-reducing research, that would otherwise be passed on to price-inelastic final demanders.

Hence locally-consumed root crops remain especially under-researched, and millet and sorghum seriously so, in SSA by national systems. Such cassava research as did take place was linked to famine prevention, not to farm-level economic priorities (Anthony *et al.*, 1979, pp. 251–252). International research on cassava and other root crops, notably at IITA in Ibadan, will have major impact on smallholders only if there is capacity and priority, at national research centers, to screen (or cross) approved varieties in the light of economic acceptability in local conditions. Yet in 1976, in the whole of Africa and Asia, only Nigeria ($2.7m) and Ghana ($0.9m) spent over $50,000 on cassava research (Judd *et al.*, 1983, p. 70) — well below the salary, equipment and support costs of one scientist. Not one of the 37 commodity-specific studies of returns to research surveyed by Pinstrup-Andersen (1982, pp. 102–104) relates to a root or tuber crop other than potatoes.

(13) *Smallholders:* no data can neatly encompass the links between overall agricultural policy and lack of AR focus on smallholders. SSA is well endowed with extension, per unit of research (Judd *et al.*, 1983, pp. 5–6); the usual problem is absence of "smallholder-friendly" research findings to extend. One reason is research attitudes, typified by the belief that "even where no formal investigations of cattle rearing have been carried out, local knowledge has frequently been acquired through the management of government herds" (Anthony *et al.*, 1979, p. 260). No wonder that researchers know so little about practices, priorities, or profitability among small herders. Frequent attempts to get smallholders to adopt fancy, complex, multi-purpose animal-drawn equipment (tested for mechanical, but seldom for economic, efficiency on large farms near good mechanics) also illustrate the divorce between much research and smallholder practice.

This partly reflects colonial hangovers in AR. Before 1939, "where a major research program was carried out on a food crop, for example, wheat breeding in Kenya and maize breeding in Southern Africa, it reflects the importance of these crops to commercial farmers" (*ibid.*, p. 251). By 1973, maize hybrid diffusion in Kitale, Kenya (as earlier in a few areas of Zimbabwe, Malawi and Zambia) had achieved "widespread adoption by smallholders as well as large commercial farmers" (*ibid.*, p. 255 and footnote). The dependence of maize, which is open-pollinated, on timely, credit-linked, annual hybrid seed distribution, however, restricts acceptance of hybrids in countries with unreliable seed supply or distribution systems. The relatively late and patchy attention to composites again illustrates the distance between much AR in SSA and the needs of small farmers.

Moreover, a "smallholder-friendly" research system must respond quickly to new types (or strains) of pest, weed and disease, and must be able to select and release new, resistant varieties accordingly. The strength of the Philippines-plus-IRRI system, in selecting replacement varieties for IR-22 in one season after its vulnerability to tungro had been shown under field conditions, has few counterparts yet in SSA, but is needed, if small farmers are to have confidence in HYVs. These seldom need much "better" farmer management than traditional varieties; but HYVs are risky in the absence of smallholder-responsive management of AR. Also, points made previously (stress on mainly subsistence crops like cassava and sorghum, on farm-level economic criteria, etc.) apply with special force to smallholder-oriented AR. Its success, therefore, depends on a less urban-biased and therefore less surplus-extractive set of agricultural policies.

6. RESEARCH: REFORM "WHATEVER THE POLICY?"

(a) *The need for formalized research design*

Given that agricultural policy is likely, in most countries (EC as well as SSA), to remain self-contradictory, pressure-ridden and messy, can anything useful be said in general about AR? Some AR-specific, almost policy-neutral, conclusions do emerge. They are, however, unlikely to be useful as parts of an organizational research blueprint — a global guide to the Ideal Form for AR. There is, unfortunately, more than a hint of such Platonism about some otherwise excellent reports on African national systems (e.g. Taylor *et al.*, 1981, p. vii; Contant *et al.*, 1982, pp. 51, 67–68; Antoine *et al.*, 1983, pp. ii, v, 43).

AR's *content* can change cost-effectively — by crop mix, via farm-level socioeconomics, etc. On its *form*, no great global verities (more out-stations for outreach, or fewer for critical mass?) are on offer; "office management" remarks (about how to reduce administrative load, how to ensure that scientists and technicians are contented yet involved, etc.) can be useful and non-obvious to directors of recently independent and obviously non-functioning libraries or filing systems or transport pools; but can something more interesting be said?

Government willingness to spend more on AR depends, in part, on the perceived efficiency of

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such spending. Suppose that "efficiency" means contribution to net value added by farm output per unit of input, irrespective of distribution or sustainability (for thoughts on how to relax this assumption, see Lipton, 1988). Also, assume that we know the relative product and input prices at the time when the research comes onstream. The "efficiency" of research can be increased either by lowering cost or by raising benefit. The resulting cost/benefit ratio is increased by delay, and subjected to risk, at each of three stages: (a) between initiation and successful output of the research (itself divisible into research and testing sub-stages: Pinstrup-Andersen, 1982, p. 50), (b) between output and adoption by the farmer, (c) between adoption and attainment of higher ratios of farm output values to farm input values.

Good research design choices, e.g. of regions or crops for emphasis, therefore do not depend only on the research director's best-estimate probability of a successful outcome, even as modified by an economist's best estimate of the rate and extent of adoption and diffusion, and the scientist's of the yield response to various levels and circumstances of adoption. They depend also on the delay at each stage — and on the degrees of confidence attached to each of those three estimates, from which we can calculate the probability-distributions of given increases in farm output in five, seven, 10 . . . years as a result of initiating the research.

In general, there is a trade-off between accepting different *sorts* of cost, delay, or uncertainty, in respect: of achieving research success; of achieving X percent diffusion given success; of achieving the expected-value Y percent rise in yields given diffusion. Also there is a trade-off between quick results and certain results; and between speed and certainty, on the one hand, and cheapness on the other. These trade-offs carry research design implications: should one concentrate research on raising output in safer areas, on raising output of safer crops in risky areas, or on reducing risk to a given crop and area?

This is all agronomically unspecific and mathematically imprecise (faults that could be remedied; for hints, see Scobie, 1984, and Jansen, 1988), but suffices to suggest that a research director would gain by spelling out the choices through an objective function (net farm value-added?), constraints, and a three-stage maximizing process in this way.[31]

(b) *Research priorities and reducing poverty*

So far, this discussion has largely omitted distributional considerations, for three reasons. First, intra-rural distribution in most of SSA is far less unequal than in South Asia. Second, poor people's main hope in SSA is to reduce the grotesque rural-urban gaps. Third, the overriding need for poor people *in SSA* is to get per-person food availability rising somehow in a set of environments where it has been falling. In SSA, unlike Asia, most poor people are directly farming for food, so that, unless unwise tractorization policies are adopted, higher smallholder food output will accompany higher capacity, among the hungry, to purchase food.

However, HYVs' "second-generation problems" arrive fast, even in SSA, especially alongside growing labor gluts. For example, the Chilalo Agricultural Development Unit (CADU) in Ethiopia "rapidly expanded use of improved varieties of wheat and teff and of fertilizer by farmers participating in the project, with an approximate doubling of wheat yields" (Anthony *et al.*, 1979, p. 256), but it thereby encouraged the dispossession of tenants by large landlords, who then displaced labor with combine harvesters (Cohen, 1975). The view that — in Kenya, for instance — subsistence-commercial "dualism is gradually being transformed, with the division of large farms in the high-potential areas, into smaller commercial farms and the formation of a continuum from subsistence farming to highly commercialized large-scale farming" (Taylor *et al.*, 1981, p. 4) is too sanguine. Political and economic factors, alongside rising person/land ratios, can induce polarization; and, even if there is a transition from bimodal to unimodal agricultures as Taylor and his colleagues predict, the result need not be less unequal, or better for the poor, if the variability of farm size (or net income) around the mean is very high in the unimodal situation.

Although the current Kenyan Plan's "basic strategy for development . . . is the *alleviation of poverty*, throughout the nation," poverty-orientation played no part in the terms of reference for the report on its AR commissioned by the government from ISNAR (*ibid.*, pp. 1, 10; their italics). Indeed, "There was no convincing evidence that major emphasis is being placed on the development of production technologies for the small farmer" (*ibid.*, p. xii). Regional distribution of research often intensifies this neglect of small and poor farmers, as in the AR priority given to the Center and Southeast in Burkina Faso (Antoine *et al.*, 1979, p. 39) and to Eastern Province in Zambia. The relative weakness of food-crop research, especially for cheap calorie sources consumed locally, further militates against equal distribution, even relative to initial income, of AR benefits. Assuming producers retain all AR benefits, such biased AR makes the rich richer, and does little for the poor. Some earlier advice about research into mechanization, and even into hoe-animal ploughing options (Anthony *et al.*, 1979, p. 259), would today make the poor poorer; the person/land ratio is higher, landless labor a more important main income source, and "labor shortage" (or, rather, price-inelastic supplies) less important *even seasonally*, than when such research was first mooted. Such factors will indicate labor intensity — on grounds both of efficiency and of poverty reduction — even more forcibly if, and when, research now being designed "delivers" to farmers.

Many sorts of change that would be introduced into AR in SSA for reasons of dynamic efficiency (a shift to labor intensity, to under-researched root crops and subsistence cereals, to risk-reduction via controlled water supply) would tend to improve income distribution as well. The goal of reducing poverty, therefore, strengthens arguments for AR reform that are strong already.

One possible exception concerns the regional balance of research. It is not always right to contrast "smallholder farmers" with those in "high-potential areas" (Taylor *et al.*, 1981, p. 31), but urban and overseas contacts do tend to polarize farm size, and in advanced areas incomes even of farmworkers are pulled up. The poor are often the remote. A research-station strategy to maximize expected net agricultural value added from given research inputs could mean further neglect of remote regions, and hence of many of the poor. If research stations need to avoid risk by going for some fairly safe "winners" among their AR projects, then initial researcher ignorance of many remote areas might direct AR away from seeking to benefit poor people there.

Even here, great gloom would be misplaced. Remote areas, because neglected, may offer especially attractive initial returns to research. Eastern Uttar Pradesh, while hardly "remote" in India, was long regarded as agriculturally hopeless, until just a little well-directed hydraulic-agronomic research brought a rich harvest of tubewell-supported HYVs. In Botswana, the northwest (Ngamiland), with good but unexploited access to surface water in many places, is quite wrongly seen as an area of "bad farming practices" by many observers from the eastern heartland, but in fact offers a bright future to research based on improved security of water supply. In general, the great threat to effective research for "backward regions" is that they get opened up (by heavily-subsidized or "free" transport and other grid-based infrastructures) to product competition — and emigration — before they have developed potential surpluses for specialization and exchange.

(c) *The massive scope for improvement*

National agricultural research in SSA is often not cost-effective. It concentrates heavily on a few export crops in price-inelastic demand, where, if it succeeds, the gains go largely to Western consumers. Poor people's crops — especially roots and cheap cereals where on-farm or local consumption (plus hunger) mean that there is little or no problem about inelastic demand — are generally neglected. Unrewarding career structures mean rapid turnover, and this plus the large number of research stations mean generally below-critical scientific masses. Yet, for all the special problems of SSA's numerous micro-climates, a reserve of internationally researched, seed-fertilizer-based, innovations is ready; but it can be relevant in SSA only if national AR can undertake breeding (though sometimes screening suffices), testing, and adaptation to local smallholders' economic circumstances.

This usually requires policy change transcending AR. Reform of AR can help, though a centralized blueprint is not a panacea. However, a context in which SSA governments drastically raise the share of domestic cash and skill resources, current and capital, for the agricultural sector (including controlled water supply, especially micro-irrigation) is needed for major improvements from AR. These will do best, not "only" for distribution but also on plain efficiency grounds, if they increasingly stress labor-intensity, smallholding, and roots and cheap cereals.

NOTES

1. Wherever possible — and always, unless otherwise stated — we have excluded data for the Republic of South Africa (RSA) and North Africa (Egypt, Libya, Tunisia, Morocco, Algeria).

2. A useful discussion using only three zones (forest, savanna, highland) is Anthony *et al.* (1979, pp. 119–128).

3. Price distortions impeding food production have become rather less serious since the mid-1970s in most of SSA (Ghai and Smith, 1987).

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4. For evidence that outlay on extension generally increases the return to outlay — especially subsequent outlay — on research, see Evenson and Kislev (1976). On SSA's exceptionally high extension-research ratios, see Oram and Bindlish (1981, pp. 44, 100); Boyce and Evenson (1975, pp. 3–13) (appropriate allowances for RSA and North Africa can be made from pp. 170–183); and especially Pinstrup-Andersen (1982, pp. 66–67).

5. There appear, however, to be diseconomies of scale to expansion of a country's *total* researcher establishment: see Boyce and Evenson (1975, pp. 99–100), who suggest that "diseconomies to the system could well be the result of rapid expansion of the numbers of experiment stations" as researcher numbers increase.

6. *Ibid.*, p. 46. The last figure includes RSA, but its ratio appears to be closer to 1% than to 1.5% (*ibid.*, p. 179 and fn.; GDP in agriculture from *South African Statistics 1982*, p. 21.6; $–R. exchange-rates from South African Reserve Bank, *Quarterly Bulletin*, No. 122, December 1976, S.62). Hence the ratio for southern SSA proper is even higher than 1.45%.

7. Boyce and Evenson (1975, p. 46). Other (generally richer) developing regions: West Asia, 0.83%; North Africa, 0.72%; temperate South America, 1.29%; tropical South America, 1.03%; Central America and Caribbean, 0.71%.

8. This can be inferred because "low-income South Asia" in Table 1 includes the same five LDCs, assigned to "South Asia" in Table 2, yet assigns these two countries over twice the scientist numbers in 1980.

9. Thus Oram and Bindlish (1981, p. 18) suggest that scientist numbers in six West African countries rose from 915 in 1971 to 3,239 in 1975, and fell to 1,897 by 1980.

10. The private sector is estimated to contribute only some 3% of agricultural research outlays in developing countries of Asia and Africa (Boyce and Evenson, 1975, p. 77).

11. Itself higher in SSA than in Asian countries at comparable income levels. For example, in the eight "SSA low-income countries" in Table 1 (Burundi, Kenya, Madagascar, Senegal (sic), Sudan, Tanzania, Togo and Zaire: Oram and Bindlish, 1981, p. 89), the proportion of their total population dependent mainly on agriculture is above 80%, as against some 65–68% for the low-income South Asian countries.

12. Standardized publications are explained, and data given, in Boyce and Evenson (1975), pp. 34–42, 84–96.

13. Though the proportion of PhD workers in AR who are not indigenous appears to be much higher, relative to the proportion of less qualified scientists, in SSA than in other developing areas.

14. Apart from the static arithmetic, there is a further drawback, if critical mass depends on continuity in, as well as on numbers at, a station. Each new station represents a new prospect of transfer, and thus, given the size of the national cadre, a new threat to continuity at the old stations.

15. If tea and coffee land (and other resources) could cheaply be shifted to other crops of similar value, this would not apply. Alas, such shifts are very costly for tree crops.

16. All the Boyce-Evenson (1975), Judd *et al.* (1983), and York *et al.* (1977) data are for *gross* agricultural product in these estimates. Logically, the Boyce-Evenson argument requires *net* product to be used. In SSA (but not elsewhere) this probably makes little difference.

17. If such crops are very labor-intensive even on estates, one might nevertheless make a distributional case for high research/output ratios — unless benefits were largely transferred to (price-inelastic) foreign demanders.

18. Proportion of research scientists working on animal husbandry, unweighted average: seven South and Southeast Asian LDCs, 9.3%; seven in SSA, 22.3%; seven in Latin America, 21.1% (Oram and Bindlish, 1981).

19. If congruence is sought, local self-consumed vegetables may well loom larger in these research budgets than marketed cabbages.

20. It is now commonplace that, for decades, AR in Kenya, Nigeria and elsewhere was conducted almost entirely on pure-stand trial plots, and conveyed to farmers as a message to avoid mixtures; and that farmers were right to mix — indeed, that the practice (where used) is usually overdetermined, raising expected profit *and* reducing risk.

21. Rudolph (1967) is a relevant text.

22. A good example is economists' success in dissuading ICRISAT from misdirecting major inputs of skilled time — and scarce land — towards high-lysine varieties of millet and sorghum. The proposal originated from experimental evidence that rats, etc., died of lysine deficiency on millet-only diets. It could, however, be shown that poor humans in millet and sorghum areas were often deficient in calories but hardly ever in lysine.

23. "The CIMMYT procedures are close to the rapid and cheap end of the collection and analysis continuum, with a turnaround time of two to three months for any one target group of farmers, compared to 12 to 24 months for frequent-visit data collection and programming analysis." This is in part a praiseworthy attempt to meet FSR needs via "manpower commitments of two adaptive AR professionals per 80,000 farms [as is feasible] in East, Central and Southern Africa," and to recognize that often "the sophisticated methods of data collection and analysis . . . are not

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cost-effective and useful in serving" smallholders there (Collinson, 1982, pp. 45–47). But it is very risky for rapid rural appraisal (RRA) — correctly designed and presented (Chambers, 1980) as a quick familiarization technique for busy politicians, civil servants and donors — to be upgraded to a research technique like this. RRA misses seasonal effects, in which decisions inside the observed two to three months interact with decisions and outcomes outside that period. Quick visitors, too, cannot expect reliable information about credit, tenure, and other forms of rural differentiation, which is increasing with land scarcity and thus rendering RRA results less and less reliable now, or extrapolable later. Many questionnaire items (e.g., Collinson, 1982, p. 54) appear to subject farmers to difficult memory tests, which will be hard to verify in a two to three month appraisal.

24. Despite the temporary effect of (b) as a set of measures to "reduce stocking rates" (*ibid.*, p. 15).

25. Collinson correctly states that FSR considers "how a farmer allocates his scarce resources . . . between crop, livestock, and off-farm production" (p. 3), but (a) in practice the latter is often left out; (b) if the entire allocation problem is to be handled, FSR turns into activity analysis and surely requires a year-round appraisal.

26. FSR is sometimes presented as OAR, sometimes as activity analysis. One recalls Molière's Monsieur Jourdain, who is amazed to learn that he has been speaking prose for 40 years. Have competent agricultural economists been doing FSR all their lives?

27. FAO (1983), pp. 11–12 and Tables 15–19, 22–25, 28, for data in this and the next paragraphs. "Market LDCs of South and East Asia" are Bangladesh, Bhutan, Brunei, Burma, East Timor, Hong Kong, India, Indonesia, Republic of Korea, Laos, Macau, Malaysia, Maldives, Nepal, Pakistan, Philippines, Singapore, Sri Lanka and Thailand (population in 1982 1.29bn, 61% principally dependent on income from agriculture: *ibid.*, p. 71). "SSA" is Africa excluding Egypt, Libya, Algeria, Morocco, Tunisia and South Africa; we have included figures for the Sudan in the SSA totals. We deduct, from "paddy" and "total cereals," 33% of output data for "paddy," to allow for an assumed milling yield of 67%.

28. This does not contradict earlier remarks about irrigation priorities in SSA. Effective spread of HYVs and fertilizers in this part of India was probably concentrated on sub-regions with more reliable rainfall than most of semi-arid SSA, not on the really risky tracts.

29. *Long-run* aggregate price-elasticities of agricultural supply in nine SSA countries range from 0.07 to 0.54, average 0.21, with six below 0.17 (Bond, 1983).

30. A hectare of soybeans yields more net value-added than a hectare of cassava; but seldom by a factor of 57 to 1, as would be required (assuming "plasticity of nature") to justify the all-SSA incongruence in research-per-hectare. Of course, the cassava area and output data are very weak.

31. Perhaps a national AR program, seeking to convince smallholders and governments of its worth, should not always maximize. It may require one striking and fairly rapid success — even at an expected rate of return of 3% — and should then therefore put major resources into a crop grown under reliable water conditions. But let us know the output we expect to lose from such an argument!

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